

should be called *Uniformis* G. & R., without any reasonable doubt. I will not enter here into the question of the even banded forms, *Buffaloensis* and *Floridensis*. The larva of the former is described by Professor Lintner. The latter has not been examined in sufficient quantity. I think now that both these forms are distinct, certainly sufficiently so as to merit a distinct title, if not of specific value. *Fuscicaudis*, Boisd., is, from the markings of the abdomen, certainly a distinct species. After myself examining specimens I can come to no other conclusion.

Quite similar arguments show that we are justified in calling the smaller of the two forms of *Arctia*, which have buff striped, black fore wings, and red, spotted secondaries, by the name *Saundersii*. I carefully considered Kirby's description of *Parthenice* after my discovery that two species were passing as *Arctia virgo* in collections. The character which separates the two is the narrower ochre veining of *Saundersii*. Again as with the *Hemaris*, Kirby fails to compare his species with *Virgo* Linn., which it was his business to know had he intended to describe as closely allied a species as *Saundersii* is. I pass by the probably correct surmise of Mr. Hy. Edwards that Kirby describes an unimportant variety of *Virgo*, assuming this author to be correct. But be this as it may, I am the first to scientifically separate the forms, to carefully describe them, to figure them side by side (at my own expense, which was a good deal in those days), to give the essential characters by which they may be known and named henceforth. This being so, it is inconsiderate, and, scientifically speaking, an erroneous view which would overturn this real work and restore a name which can never be proved to be correctly applied. Always there must be a doubt hanging over the name *Parthenice*, from Kirby's own fault in ignoring *Virgo*. But there is no doubt about *Virgo* or *Saundersii*. I cleared those doubts up years ago, and Mr. Hulst should have profited by my work rather than attempt to re-introduce the uncertainty which I had, I think properly, dissipated.

ELAPHIDION VILLOSUM, FABR.

BY FREDERICK CLARKSON, NEW YORK.

In support of the record relating to the periods of transformation of this beetle, and the probable cause of their pruning the branches of the Oak, which I had the pleasure to contribute to the XVII Vol. of this

journal, I now add some further facts, resulting from a recent visit to Clermont, N. Y.

On the 29th of October I gathered from under a group of *Quercus tinctoria*, seven branches that had been pruned by this longicorn. The tunnels were from ten to fifteen inches long, in branches from one-half to three-quarters of an inch in thickness. The branches I carefully divided lengthwise, so that the parts could be replaced in position. Six of them contained the pupa, one the larva, which pupated November 4th. One of the pupæ I preserved as a specimen. The imagines appeared on the following days: Nov. 14th, 22nd, 26th, 29th, Dec. 9th and 25th, all females.

These transformations were rather hindered than advantaged by meteorological conditions, for they occurred in a room having a northern exposure, in which, during the period of the transformations, the thermometrical record differed but little from that in the shade without. Had the branches remained upon the ground, the included insect would have received all the benefits resulting from the direct rays of our Indian Summer's sun, as well as the moisture from the ground; influences that ordinarily assist development. As the imagines appeared they were examined and replaced in their tunnels, where they now remain in a passive state, and not likely, I think, to exhibit their natural activity until next May or June.

The object of the paper referred to, as well as this article, is to present facts that seemingly disprove certain theories relating to the habits and metamorphoses of this beetle, which have been formulated by distinguished sires and accepted by their credulous sons. What Drs. Peck, Fitch and Harris have written upon this subject has been substantially repeated by almost every entomologist who has undertaken a history of this beetle. We are very apt to fall into line when we have an abiding confidence in a leader. While I am unwilling to deny the conclusions of these naturalists, I yet think that the facts related go to show that the insect matures at a period earlier than that named by them, and that the benefits supposed to result from the dismemberment of the branch, in so far as the changed environment is concerned, are wholly unnecessary to the development of the included insect, and that there is a plausibility in the inference, if not a certainty as to fact, that the object of pruning the branch is to prevent the flow of sap. If the habits of this beetle as given by these doctors are to be regarded as *ipso facto*, then we must admit the

possession of a faculty in these lower organisms that towers above instinct and presents the feature of intelligent reason. This is a subject that cannot very well be discussed in these pages, yet it may not be out of place to say that able writers on the question very generally admit that the habits of insects follow a prescribed law, by some regarded, in a materialistic sense, as mechanical; and by others, spiritually considered, as in furtherance of a divine edict. This latter view is very cleverly presented by St. George Mivart, in *Organic Nature's Riddle*: "Our experience," he writes, "is in favor of the existence of an intelligence which can implant in and elicit from unconscious bodies activities that are intelligent in appearance and result 'Unconsciously intelligent action,' improperly called 'intelligent,' is that which is called intelligent only as to its results and not in the innermost principle of the creatures which perform such actions." "Instinct," Todd says in his *Cyclopædia of Anatomy and Physiology*, "is a special internal impulse urging animals to the performance of certain actions which are useful to them or to their kind, but the uses of which they do not themselves perceive, and their performance of which is a necessary consequence of their being placed in certain circumstances."

If such definitions are accepted, how are they to be reconciled with the marvellous statement as given by Dr. Fitch? That the larva should prune the branch to prevent the flow of sap would be a necessary consequence of its being placed in certain circumstances, but to do so that the branch may fall to the ground presents a course of reasoning that relates to a condition foreign to the then existing environment. The habits of this beetle from the period of egg-hatching, as given by Dr. Fitch, displaying as it did to him extraordinary intelligence, impress me as presenting the most natural instinctive qualities. The ova, he says, is deposited on a small green twig, the soft pulpy tissues of which nourish the infant larva, which when increased in size and strength, attacks the hard wood of the branch, transversely, in a circular direction, consuming it all, leaving the branch supported only by the bark. From these premises, without pursuing the subject further, it is evident that the infant larva requires sap-wood for its sustenance, which it derives from the twig, but so soon as its strength permits, it seeks for dead-wood by attacking the branch, which is found more and more free from sap as the work of severance progresses. The aim therefore from the start is to obtain the dead-wood,

and when the branch is eaten through the larva continues its feeding in forming a tunnel through that portion of the branch which is cut off from the supply of sap.

The instinct of insects is wonderful enough, and more accurate perhaps than a mental process, but while we justly ascribe to them all the attributes pertaining to their natural gift, we are not warranted in imputing to them an intelligence only to be arrived at through a course of reason.

BOOK NOTICE.

THE BUTTERFLIES OF NEW ENGLAND, with original Descriptions, accompanied by eight lithographic Plates, in which are given at least two hand-colored Figures of each Species. By C. J. Maynard. Boston, Bradlee Whidden, 1886.

The author of this work is a well-known taxidermist and ornithologist, who here endeavors "to present recognisable figures and intelligible descriptions of the Butterflies," etc. The drawings and descriptions, he says, are original, but he has been obliged to compile descriptions of the larva and pupa from works of various authors, and is indebted to such works for many notes upon habits, distribution, etc.

He has coined a new set of names for the veins of the wings, and likewise a set of English names for the several species. No authority follows the specific names in the text, but those "who are curious to learn who first described any given species, can ascertain by referring to an Index," etc. He sees no reason why polymorphic forms, which he calls "phases of coloration," should receive names, and in this connection thinks that entomologists have overlooked the results of an important law, that of reversion. The classification adopted is that used in the arrangement of the collection of the Boston Soc. N. Hist., "merely because of its simplicity," and hence the series begins with the Satyridæ, and Papilio stands next the Hesperidæ. By all which it will be seen that the author has ideas of his own, but is not very well read in the recent literature of lepidopterology.

The plates are described as hand-colored, and it seems to be implied that the coloring therefore is something superior. Whereas it is very bad indeed—could not well be worse. This is conspicuously so in *Limenitis*,